METHODOLOGIES FOR CONDUCTING ON-FARM LIVESTOCK RESEARCH WITHIN MIXED FARMING SYSTEMS

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Introduction

Farming systems research methodologies were developed initially with a focus on agronomic trials. It was not until recently that researchers recognized the importance of including animal studies in farming systems research. The methods used for conducting on-farm livestock research (OFLR) were developed from experiences with cropping trials. But because of unique problems and constraints, many crop research methodologies proved inadequate. The design of animal experiments may be doubly difficult because the variables associated with agronomic on-farm research are often involved in forage production and combine with the problems of animal experimentation. Research with large animals limits the number of replications because of their high relative cost, diverse genetic background, variation in age, condition, and past nutritional history. The science of biometry developed from agronomic research was slow to be adopted by animal research centers. Its application to OFLR presents difficult problems. Likewise, the lack of communication and interaction among animal scientists conducting on-farm trials with livestock suppressed the dissemination of experimental methodologies. As a result, conferences on research methodology were held in Ethiopia, Syria and Togo to bring together researchers and develop guidelines from past experiences. This paper is based on topics and methodologies presented during those meetings.

The purpose of this paper is to present methodologies considered important by researchers when conducting OFLR. The areas discussed include the selection of the research site, farm selection, farmer participation, experimental design, data collection and the analysis of research results. These topics do not represent all the components of on-farm testing using the farming systems approach. However, they reflect areas from past experiences which were emphasized and well documented.

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Research Site and Farm Selection

The selection of a research site and subsequent participating farmers should result in an area where the research results are transferable with some degree of predictability to a larger area (Thomas et al., 1982). The site selection process should define a group of farmers with similar livestock and husbandry practices. If major differences exist between regions, more than one site can be selected. Without defining a homogeneous group of farmers for making recommendations, research results may not be applicable or could even be damaging. Shaner et al. (1982) has described criteria for selecting research areas. The representativeness, size, accessibility, and closeness to an experiment station of the area and the cooperation with farmer agencies are discussed. Experience has shown that when conducting OFLR, additional criteria are needed to define an acceptable site.

Site selection criteria. In selecting a research site, villages or areas must be stratified on the basis of criteria important to the research team. However, not all criteria are directly related to defining distinct populations. Bunderson et al. (1985) suggested that the region should be a major production area, have a high population density and the agriculture sector must have potential to develop. Resource availability, ease of access,

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available baseline information, and village leader cooperation are also important (Ahmed et al., 1985; Thomas et al., 1982; Bunderson et al., 1985). The use of parameters which reflect livestock populations and management can stratify farmers into discrete groups. The region's physical characteristics are important, for they directly influence agricultural practices (Bunderson et al., 1985). Agricultural activities are the primary criteria used in many site selections (Thomas et al., 1982; Bernsten, 1982). The strong interaction between crops and animals suggests that grouping farms with similar agronomic conditions and practices will produce groups of homogeneous farms based on animal husbandry practices. Often it is assumed that farms which have similar feed resources produce similar animals. However, Thomas et al. (1982) found this not to be true when conducting studies in west Java. Animal management systems (e.g., traditional or semimodernized), and the mean animal population density have been used to stratify sites according to animal parameters (van Eys et al., 1985; Ahmed et al., 1985).

Because some of the criteria suggested are highly subjective they become difficult to prioritize according to their importance. Thomas et al. (1982) suggests that because program development depends on institutional cooperation, it is highly unlikely that research sites can be selected by objective criteria alone. A combination of subjective and objective criteria must be involved in site selection.

The data obtained from baseline surveys should provide information about the relationships between the animal production system and the physical and socioeconomic environment. By using the pertinent criteria, sites and farmers are differentiated into homogeneous groups. However, the criteria selected may not always produce distinct classes. A study described by van Eys (1985) used agricultural activities, animal management, mean animal population density, and leader cooperation for village site selection. Farmers were selected from the baseline survey using a proportional, stratified random sampling technique with land ownership as the main strata and animal ownership as the substrate. The effectiveness of this methodology was studied by Thomas et al. (1982). It was found that sheep and goats performed the same at each site in terms of growth performance, even though there were differences in structure and management of the small ruminant enterprises between sites. The research sites were not different with respect to growth parameters as was originally thought. Farm size or ownership is used often in agronomic studies and is known to influence cropping patterns (Thomas et al., 1982). Farm size was used as a criterion for this study because it was assumed that size would determine feed resources and affect growth variables. However, Thomas found that there was no relationship, as feed was purchased off-farm. To improve the stratification of populations, he recommended that initial surveys be used to provide more information about the system so farms can be grouped "on the basis of parameters found to be quantitatively related to the enterprise under study." In addition, he recommends that the quantitative parameters related to the biological factors of the farm should be measured over time to describe better a dynamic livestock system.

Sampling methods can also affect site and farmer selection. The use of random sampling can create the risk of drawing from different populations (McIntire, 1986). If sampling is done from volunteers, homogeneity may be eliminated by the selective bias of collecting information from progressive farmers only. Sample size is affected by cooperation. Ideally, a large number of randomly selected farmers result from a baseline survey. However, the opposite is usually the result. A trade-off occurs with random sampling and cooperation (Tully et al., 1985).

Farm selection. The criteria for selecting farms to participate in OFLR are again based on the goals of the research team. The farmer must be able to supply the species and number of animals needed throughout different seasons and length of the trial (Ahmed et al., 1985). This becomes very important when conducting trials which collect data for more than a year (e.g., reproduction). Animal ownership is an important criterion (Gryseels and Anderson, 1985). In some cases, animal ownership may be difficult to determine due to complex agreements between different groups and families. OFLR conducted within such systems may not be possible as owner consent is generally a prerequisite for participation. When conducting grazing and animal traction studies, the ability to provide land is important (Tully et al., 1985). In some cases, the farm must be secure from accidental grazing. Cooperation with the research team generally determines if the farmer will be used for the trials (Ahmed et al., 1985; Tully et al., 1985; van Eys et al., 1985). A cooperative attitude is considered more important in livestock trials because of the increased management changes needed to conduct OFLR (Tully et al., 1985). Certain farmer characteristics can influence their selection. Conviction of the project and willingness to try, ability to perform the test, literacy, and patience for recurrent team visits can be important (Bunderson et al., 1985; Sidahmed et al., 1985; Ahmed et al., 1985; Hadjipanayiotou, 1985). The amount of experience with previous research projects will affect farmer selection. Gryseels (1984) found that farmers with no previous research association and those who participated voluntarily were preferred for OFLR. These farmers were interested in the technology and participated for the length of the trial. Also their evaluations were not biased from experiences with past studies. A trade-off occurs with using volunteers and selecting nonexceptional farmers (Tully et al., 1985). Volunteers may often be the progressive farmers and not representative of the population (see site selection criteria). In contrast, Modawi et al. (1985) preferred to use farmers who had relatively more experience in the enterprise of interest. Tully et al. (1985) reported that farmers should not be closely related. Collaboration between families during a trial could increase non-treatment error by altering or exchanging assigned treatments between farmers. The farm's location, accessibility and its distance from the research center can also have a large effect (Tully et al., 1985; Hadjipanayiotou, 1985). In some regions these criteria become very important due to lack of transportation and rugged terrain.

Farmer Participation

Farmer participation is an important, but often overlooked area when conducting research. It can determine the success or failure of an on-farm trial. The way in which farmers are incorporated into the trial will influence the success of future trials as well. This section will discuss farmer participation and the use of incentives and compensation during OFLR.

Participation. As stated above, the participation of farmers can affect the outcome of the experiment. Without it, no OFLR can occur. What induces farmers to participate in an experiment? Incentives offered by the research team would appear to be a primary reason. Modawi et al. (1985) stated that cooperation brings benefits. Many types and forms of direct incentives have been used and will be discussed in the following section. Besides direct incentives, there are indirect ones that also are apparent to the farmer. In a sheep trial conducted by Hadjipanayiotou (1985), farmers that participated could sell their animals for a higher market price. It was well known that

participating farms produced sheep superior to those traditionally raised. This created more interest in the project and increased participation.

Compensation and incentives. Compensation for farmers are primarily offered to increase participation by reducing the risk and inconvenience associated with an experiment (Tully et al., 1985). This is more important when conducting OFLR than with agronomic studies. The loss of a single animal can result in a large economic loss to the farm family. Likewise, the change in management practices associated with a new animal technology are greater than a change in a cropping system. By offering incentives, the farmers gain trust in the researcher and helps insure the testing of new technologies.

The degree of compensation or incentive is a function of the degree of risk involved with cooperation. For example, a farmer would need more compensation when conducting trials on the farmer's land and using the farmer's own animals compared to trials where the farmer's personal involvement or animals is limited. A large compensation package of credit and insurance may be required when farm animals are put through unfamiliar feeding practices, while only slight technical assistance is needed when conducting researcher managed trials. The level of importance of animals on the farm and the level of investment may determine the amount of farmer involvement (van Eys et al., 1985). This can also influence the amount of compensation needed by the farmer. If the level of investment and importance are low, relatively less compensation is needed to induce participation than on a farm where the investment and animal importance are high.

There can be disadvantages when providing incentives. It may become difficult to evaluate farmers' opinions concerning a new technology, as farmers are influenced by the type and level of compensation (Hadjipanayiotou, 1985). The use of free supplements may have biased the reaction of the farmers and led to false conclusions about the technology. If the level of compensation falls short of farmers' expectations, they may reject a technology because of their dissatisfaction with the compensation and not the technology. In livestock trials conducted by Gryseels (1986), no direct incentives were offered. It was felt that any inducement to participate would interfere with a true evaluation of the rate of adoption. Farmers would participate only to receive the incentive. After removal of the incentives, farmers would drop out of the experiment.

The type of incentives and compensation offered are as varied as the OFLR. Feedstuffs are used in different ways. When new feed resources are introduced, they are generally given to farmers to test on their animals. Feeds can be sold to farmers in order to overcome temporary shortages during a drought (Gryseels et al., 1985). This not only compensates farmers for their losses, but allows the trial to continue (Nour et al., 1985). Feeds can be subsidized or given as compensation for testing technologies other than nutrition (Tully et al., 1985). For example, if certain feeding practices are being measured, compensation in the form of a feed already being fed could change the management practice under study. Tully suggests that if farmers are feeding barley, then compensation should consist of another feedstuff such as wheat. When introducing new species to a region, farmers are generally provided with animals. To ensure participation throughout the trial, some form of agreement is necessary. Goats were introduced to a farming system and given to farmers as incentive to participate (Sidahmed et al., 1985). After one year on the experiment, farmers took ownership of the goats. In another goat trial, farmers received one mature goat or sheep with the stipulation that they return three postweaning offspring over a five year period (van Eys

et al., 1985). Implements are offered when introducing animal traction (Gryseels, 1985). During the introduction of single oxen power for crop cultivation, plows were sold to the farmers. Production inputs can be used to support a new technology. With the introduction of goats, participating farmers lacked the necessary milking equipment (Sidahmed et al., 1985). It was supplied to the farmers in addition to vaccinations and disinfectants. While all were available locally, they were given to farmers as incentives. Free services have been used with different trials. Although farmers enjoy this type of incentive, there may be social constraints which limit its use. Bunderson et al. (1985) found that farmers became suspicious that "nothing goes for nothing." As a result, he recommends that a nominal fee be charged for all services and inputs. Of all the incentives mentioned above, cash has the greatest appeal. In particular, it has been used for payment of field labor, use of land and for housing technicians (Sidahmed et al., 1985). It can be used to insure against losses during the trial and given in the form of operating loans (Gryseels et al., 1985; Hadjipanayiotou, 1985; Sidahmed et al., 1985). Other types of nonconventional incentives have been used with some success. A guarantee can be given to farmers to insure the return of a collected sample. This becomes important when measuring milk production (Sidahmed et al., 1985). Hadjipanayiotou (1985) used organized competition between participating farmers. This helped promote interest in the project by creating friendly competition and publicly demonstrating the advantages of using the new technology.

Experiences from OFLR have shown that farmer participation is an important factor in the success of any trial. Incentives, either direct or indirect, are needed to compensate farmers for the risk and inconvenience. The amount and type of incentive are functions of the amount of riskiness and type of trial conducted. The higher investment in livestock and lower numbers of animals makes OFLR more risky than agronomic studies. Work with cattle demands more compensation than studies with smaller ruminants. Negative effects can arise from using incentives. These should be recognized at the beginning of the trial so that an increase in farmer participation does not come at the expense of unbiased information.

Experimental Design and Testing

During the testing phase, the research team will design trials which measure the performance of different technologies, compare new technologies with traditional practices, and identify new resources and production constraints. Studies can be oriented to test for significant differences, the adoption of a technology, basic research or allow farmers to experiment independently (van Eys et al., 1985; Okali et al., 1985). The most prevalent method is to measure productivity through control of certain experimental parameters and test for differences by using various statistical models. This generally occurs when conducting researcher managed trials. It has been reported that tests for significance are difficult to produce with OFLR (van Eys et al., 1985; Gryseels and Anderson, 1985; McIntire, 1985; Petheram et al., 1985). There exists a high risk of confounding treatment effects with environmental effects due to large variations between animals and farms. This confounding leads to inefficient estimators of treatment effects and decreased confidence in making recommendations (McIntire, 1985). Other problems with testing include no within farm replications, no control group, few animals, multipurpose animal output, and environmental effects that increase nontreatment variation (Gryseels and Anderson, 1985). This section will discuss methodologies and experiences in experimental design, experimental units, replications, and data collection.

Alternative testing procedures. The majority of testing with livestock is oriented towards using statistical models and tests of significance to evaluate innovations. Because small differences in treatments are difficult to detect, on-farm testing should not be used for basic research (Bernsten, 1982). Animals are difficult to pair and replicates are few. These factors, so important in basic research, can be better incorporated into on-station trials. To detect such small differences on the farm would require a large amount of control. If much control is needed, the technology would probably never be adopted by the farmer (Bernsten, 1982). Because of the difficulties mentioned above, alternative testing methods may be appropriate. When extreme variation exists between farms, Zandstra (1985) suggests that surveys can be used to gain insight and information about the system without testing. Similar methods are used by farm management and social science professionals. A second alternative, allowing farmers to experiment with a new technology, is seldom used by researchers (Okali et al., 1985). Atta-Krah (1986) reported the use of farmer experimentation in the introduction of alley farming. Farmers were given seed and advice on how to utilize fodder trees. The farmers decided how best to incorporate the technology into their system. This method of evaluation provides information on which innovation they perceive as beneficial and directs future research. Okali et al. (1985) outlines the methodology for utilizing this form of testing. A new technology is implemented on an animal or animals followed by a visual evaluation of the difference between treated and control animals. This assessment would then be combined with past experiences or alternative technologies. The farmer would also evaluate how it affected other areas of the farm. Farmers' perceptions would form the basis for accepting or rejecting the technology.

Experimental design. Harvey (1986) has outlined two basic experimental designs which are applicable to OFLR. The first concept deals with a cross-classified design where treatments are compared with farms. Here, all treatments are tested on the same farm. Farm and treatment effects are considered as well as other sources of variation. Age of animal, sex and time of measurement are but a few sources of nontreatment error. Generally, this type of experiment is not possible when testing large ruminants. Farms lack sufficient number of animals to test all treatments. The problem with small numbers of animals is not limited to cattle. In some regions, there may be only two or three sheep and goats per farm (van Eys et al., 1985). Where large flocks of sheep or goats exist, this design may be considered. When enough animals are available for testing each treatment, the question arises as to whether it is more beneficial to increase the number of animals tested per farm or increase the number of farms. While testing feedlot cattle Ahmed et al. (1985) preferred to use more animals and fewer farms. They found that by increasing the number of farms, a more representative sample was obtained but travel increased significantly and fewer treatments could be tested. Their objective was to obtain as much information about numerous treatments as possible.

The second design discussed by Harvey involves the application of a single treatment per farm. Farms are randomly chosen to test each treatment which nests farm effects within treatment effects. When pairing farms, farm effects will be large, resulting in the need to use many more farms than in the first example. This design tests acceptability of new technologies but is inefficient. The use of different farms for replicates confounds treatment effects with farm effects (Sands et al., 1984). This limits precision and makes treatment differences difficult to detect using standard levels of significance (van Eys et al., 1985). To improve precision, it is recommended

that the number of treatments be limited while increasing the number of farms. The decision to use either of the two models is dependent on different factors. As mentioned above, the number of animals available on each farm is a major consideration. The amount of variation between animals also affects the choice of models. If variation between and within farm animals is large, only one treatment per farm is possible. Van Eys et al. (1985) found that variability between farms was less than within farms when measuring weight gains and performance. The variability in flock characteristics, feed sources, farm management, and animal husbandry practices made more than one treatment per farm impossible.

Disproportionate subclass frequencies are common in OFLR (Harvey, 1986). It is difficult to maintain equal classes throughout the trial. However, if the statistical model used demands equality, certain adjustments can be made to account for lost observations. The following example outlines the procedure used by van Eys et al. (1985). A completely randomized block design was used with level of productivity as blocks and farm size as covariance. The criteria used for blocking were mortality, post-weaning gain, and gain corrected for litter size of goats. Equal treatment groups were maintained by eliminating farmers from the group as others voluntarily left the study for various reasons. Stability of the flock, in terms of size and composition, determined which farms would be removed.

Replication. The replication of treatments is an important component of any experimental design. It provides estimates of experimental error, improves precision by reducing the standard deviation of treatment means and increases the scope of inference (Steele and Torrie, 1980). The more replication possible, the more information available from the experiment. However, increased replication requires sampling from a less homogeneous population which results in a larger experimental error. In OFLR, replicates usually occur on different farms (see Experimental Design). The amount of variability between farms will determine the number of replicates needed to detect significant differences between treatments. While no recommendation will be applicable for all situations, past experiences offer general guidelines for treatment replication. Replications are most important when testing treatments (Zandstra, 1985). It is less important when sampling farm types. A good representative sample from the adoption domain is more effective. Zandstra (1985) also recommends that a trial begins with fewer replications and more treatments. As information is collected, the number of replications should be expanded and the number of treatments reduced. For example, when six treatments are tested, twelve farms are needed to participate. Two replicates per treatment are used. The following year, three treatments were imposed on twelve farms. An increase in the number of farms is possible as well. This same format was suggested by Oxley (1985) for evaluating the introduction of animal traction. The number of replications should be a minimum of six farms with an additional six farms representing the traditional system. Van Eys et al. (1985) found that in order to detect a 20% difference at P=.05, seven or eight farms per treatment were needed when testing nursing lambs and kids. Only four or five replicates were needed if testing was done with weaned animals. The differences in replicates were due, in part, to the larger diet effect on the preweaned animals. More variation was seen within this group than between the weaned animals. The sample size was twenty farms in a trial conducted by Bunderson et al. (1985). Ten replicates were used for each of two treatments. Farms were located in the same village to reduce variation.

Experimental units. An experimental unit is described as the unit of material where the application of a treatment is applied. As with all trials, the experimental unit is dependent on the focus of the research. Tully et al. (1985) and Petheram et al. (1985) used individual animals while Zandstra (1985) and Oxley (1985) discussed using pairs of animals as experimental material. Pairs are the best way to reduce variation, however, effective pairing of animals is generally impossible (Zandstra, 1985). Fear of new technologies, lack of animals and misunderstandings of the testing procedure cause reluctance by farmers to commit more than one animal to the trial. Flocks or whole herds can serve as experimental units. Measures of herbage intake of grazing animals or reproductive parameters may best be recorded from groups of animals. When the objective of the trial is to study farm types, the farm may be the experimental material.

The selection of animals for a trial is usually left to the participating farmer. A certain amount of bias will result with the farmer's selection. To avoid uncertainty and reduce potential losses caused by the study, the farmer may select the poorest animals. Conversely, the best animals may be chosen in order to satisfy the research team. Social or religious ideas can also affect the farmer's choice. As a result, the experimental animals may not represent the general population. While a completely random sample is difficult to obtain, the farmer's selection methods offer valuable information on the relative importance of different animals and the farmer's perceptions of the experiment. In some cases, the research team may be required to purchase animals as replacements or to do initial testing of new species. Limited availability of animals may make it difficult to acquire a sample with the proper characteristics. Fadlalla and Cook (1985) found that healthy and physiologically correct animals needed for their study were difficult to buy at local markets. The need to make unplanned, repeated trips to the market used scarce project resources. If the inherent biases and difficulties in selecting experimental animals are recognized at the trial's beginning, new information on farmers' preferences can be identified and incorporated into future experiments.

Control of environmental effects. Researcher or farmer managed trials are two types of experiments used with OFLR. The decision to use one or the other depends on the degree of control wanted. Researcher managed studies are said to provide more control, improved cost/benefit estimates and a better evaluation of potential technologies (Bunderson et al., 1985). As the farmer gains more control of the experiment, variability of most production parameters increases. Along with variability comes the increased risk of nontreatment error. Two types of nontreatment error have been identified which affect OFLR (McIntire, 1986). Covariate risk affects the whole sample or population. The effects are not generally correlated with treatments. For example, because of nonrestrictive grazing, animals eat a variety of forages and create difficulties in quantifying the effects of feeding trials (Atta-Krah et al., 1985). McIntire suggests that this type of error acts only as a source of inefficiency in the estimator and not a bias unless mobility is caused by the treatment. Therefore, no restriction of grazing is needed. Death or sale of an animal during the trial constitutes a specific effect. Such an effect on the experimental unit is generally unrelated to the treatment and cannot be controlled, thus resulting in lost observations. The effect on the experiment of a sold animal will depend on its characteristics. It could have a large effect if the animal was the best or worst of the group.

While many environmental effects cannot or should not be controlled, it may become necessary to modify the protocol in order to continue testing the

intervention of interest when conducting researcher managed experiments. Many unforeseen effects, like adverse climatic conditions, have the potential to end trials if experimental procedures or objectives remain unchanged. Likewise, changes initiated by the farmer during testing could lead to increased nontreatment error. The following examples come from experiences where modification or control was necessary in response to a specific environmental effect. While conducting feedlot trials, a drought and famine caused significant changes in meat and feed prices (Ahmed et al., 1985). Subsequently, feeding practices under study were modified by the farmers. Grain was then provided at subsidized prices so farmers could again continue their "normal" feeding practices and the trial could continue. When the researcher conducts nutrition studies, it becomes important to quantify the basal ration. However, the quality and quantity of feed is highly variable between farms and seasons (Petheram et al., 1985). During a trial, this causes large differences between similar treatments. Therefore, nonsignificance may be the result of treatment variability and not from variation in dependent variables (Petheram et al., 1985). Petheram suggests that basal rations be tested regularly throughout the trial and that farmers be grouped according to their feeding ability and feed resources. A control group must maintain their normal feeding practices in order to effectively compare treatment results. Petheram et al. (1985) found that control farmers were influenced by neighbors who were testing different treatments. They increased supplementation to their own cattle upon noticing feed changes within treatment farms. The effect of neighbors cannot be overlooked when monitoring husbandry practices. Likewise, it is difficult to know with certainty about the treatment of control animals without developing a placebo treatment (Petheram et al., 1985). One method, described by Petheram et al. (1985), recommends that the "control" group actually receives a complete supplement and the treatments are deletions rather than supplements. Variation in feeding frequency is a common problem. Petheram found that some farmers would finish their monthly allotment of feed in three weeks. One solution would be to prepare separate bags of the daily supplement. However, this would remove the ability to analyze the farmer's adaptation to a new technology. Farmers may change the composition of the experimental ration during the trial. Lactating cattle were tested for effects of hay supplementation during the dry season (Bunderson et al., 1985). To maintain milk production, farmers could supplement the ration with a locally produced seed cake when available. This practice conflicted with the study of hay based rations. To avoid conflicts with the farmers, nonlactating, nonpregnant females became the experimental unit. A trade-off between initial objectives and farmer interest occurred in order to continue the study. An experience from studies in animal traction demonstrates that farmers may substitute animals. Trials in the introduction of single oxen implements were affected when farmers substituted tired animals with fresh oxen or reverted to two oxen plowing techniques (Gryseels, 1986). Social commitments restricted field preparation on private plots. In order to insure completion of their own field work, farmers would resort to using a two oxen system. They perceived the new single ox system as inadequate before it was tested. Although these types of effects are difficult to control, they provide additional information which can be used to adjust future experiments.

Data collection. During the experiment, there is always compromise between the need to collect adequate data on experimental variables and the difficulty in doing so (Thomson, 1984). When planning the experiment, Thomson recommends that a list of variables be constructed and ranked according to their importance. Not only is researcher importance a criterion, but the farmer's criteria should be considered as well. The research team should consider variables which are noticed by the farmer in the shortest period of

time (Thomson, 1984). Rapid detection of treatment effects is essential in maintaining farmer interest and participation. Difficulty in collecting data may exclude a variable from the list regardless of its importance. Therefore, a careful evaluation of each variable should be undertaken before starting the experiment.

The collection of qualitative data during the trial is a unique characteristic of on-farm research. It involves recording farmers' observations and comments concerning the new technology. Likewise, a visual interpretation by the research staff gives valuable information on treatment effects which are difficult to quantify. Observations are generally recorded by interviewing farmers during periodic farm visits (Sidahmed et al., 1985). Farm field days offer additional opportunities to obtain farmers' insights prior to farm testing. The frequency of recording qualitative data is dependent on the project's resources and the amount of information required. Van Eys et al. (1985) found that one staff visit per month was sufficient. However, the large distance between farms limited visits to two per day. With a large sample size, less frequent visits may result. Farmers should also be notified in advance of the visit. This helps insure the farmers' presence and improves time management. As farm communication is difficult, it is suggested that a visitation schedule be agreed upon at the beginning of the trial.

Due to the nature of OFLR, most quantitative data collection procedures need modification. To suggest recommendations for each method is beyond the scope of this paper. However, several papers offer an excellent description of various methods. Detailed data collection procedures during grazing trials may be found in van Eys et al. (1985) and Sidahmed et al. (1985). They discuss measurements of various animal and forage parameters including the use of esophageal fistulated goats. Starkey and Apetofia (1986) present methods for testing implements used in animal traction. In testing mineral supplements, McDowell et al. (1986) discusses several techniques. Body weight and milk production are variables common to most livestock trials. Therefore, several suggestions for collecting data on these parameters are outlined below.

Monitoring weight changes is a common activity with OFLR. An animal's weight reflects feed intake, feed quality, and levels of work and production (Starkey and Apetofia, 1986). The variability in body weight during any point in time can be affected by gut fill. This factor makes accurate measures difficult with large animals. Several trials outline procedures which address the problems of weighing animals. Weights were recorded at the beginning and end of trials conducted by Bunderson et al. (1985) and Hadjipanayiotou (1985). During longer trials, Sidahmed et al. (1985) weighed sheep and cattle every fourteen days. To adjust for differences in gut fill, Tully et al. (1985) weighed for two consecutive days at the beginning and end of a twenty eight day period. In contrast, a minimum of four postweaning weights are recommended to evaluate treatment effects with lambs (van Eys et al., 1985). The two most common weighing methods are scales and heart girth measurements. Scales must be portable if used for OFLR (Ahmed et al., 1985; Starkey and Apetofia, 1986). Portable load cells can be used in place of scales (Starkey and Apetofia, 1985). Calibration can be done at the site, but animals sometimes resist entry. Heart girth measurements are inexpensive and easy to use. However, differences in measuring techniques between technicians can cause large weight differences from the same animal. Therefore, the same technician should measure the same herds or animals each time. Starkey and Apetofia (1986) recommended that heart girth measurements be used only in the assessment of general weight trends. At most, measurements should be done monthly or bimonthly throughout the trial. A condition scoring system may be an

alternative to the above methods. It can alleviate problems associated with gut fill and farmer inconvenience. The condition score should be farmer developed using farmers' terms (Starkey and Apetofia, 1986). For example, participating farmers in a study reported by Nour and Hamza (1985) used flank measurements, brisket size and hair coat condition as their criteria. If farmers are unable to develop a system, other experienced local people may be called upon (Starkey and Apetofia, 1986). Meat traders or buyers could develop a system using criteria important to marketing beef animals.

Because of its important economic and social value to small producers, milk production is a dependent variable for many trials. While measured to evaluate dairy production systems, monitoring milk output has been recommended as an efficient, indirect indicator of treatment effects (Tully et al., 1985; Thomson, 1984). For example, farmers can see changes in milk production faster and easier than in body weight. Milk production is difficult to quantify while animals are nursing (Hadjipanayiotou, 1985; Modawi et al., 1985). In order to adjust for suckled milk, Modawi et al. (1985) measured milk from two or three quarters to calculate total production. They assumed equal production from all quarters. However, many factors are known which cause milk production to be unequal. Physiological differences between front and rear quarters, effects of mastitis and dry quarters challenge this assumption and lead to erroneous estimations. Nursing young can be removed prior to milking. Sidahmed et al. (1985) separated kids from their dams eight hours before milking. Depending on the facilities, this may or may not be a viable method. Regression equations constructed from on-station research have been used to estimate milk production (Hadjipanayiotou, 1985). Body weights from male and female offspring taken during on-station research are correlated with total milk production from dams. Weights from on-farm offspring are then used as estimators. However, differences in growth patterns between farm and research station animals may make regression estimates unreliable. Human error also contributes to the difficulty in collecting milk samples. In some societies, milking is the sole responsibility of women and serves as their only source of cash. Fadlalla and Cook (1985) found that women milkers would remove a portion of the sample for their own use before it was weighed and recorded. To ensure an accurate weight, women were replaced with men to reduce losses from theft.

Analysis of Experimental Results

The analysis of research data collected from OFLR incorporates the use of different analytical techniques. The goals of any systems research go beyond the determination of conventional biological parameters (Lightfoot, 1984). Traditional experimentation deals with means and utilizes productivity as the only criterion for the final evaluation. To more accurately simulate the farmer's decision making process, data collected during OFLR must allow for biological, economic, social, and acceptability evaluations (Olson et al., 1986). The utilization of these four parameters allows the team to make recommendations that will hopefully have a positive impact on a large number of farmers. Unfortunately, detailed documentation of incorporating more than biological and economic analysis into the evaluation process is very limited. This section will report analytical methods used in evaluating biological, economic and empirical data associated with OFLR.

Biological analysis. Analyzing any technology usually begins with the evaluation of a biological response. The team attempts to determine if the technology produced more from a set of resources, utilized less resources or stabilized increasingly scarce inputs (Shaner et al., 1982). The difficulty

in showing statistically significant differences between treatments has already been discussed. Large coefficients of variation for most production parameters, the difficulty in indentifying linkages between parameters and the confounding of farm effects contribute to large nontreatment error terms (Gryseels, 1986). As much nontreatment error as possible should be removed by statistical methods. If the use of statistical models is required, the analysis should start with simple descriptive statistics (Atta-Krah, 1985; Oxley, 1985; Ahmed et al., 1985). These include range, means, variance, standard deviations, and standard error of the means. Such parameters are derived from the comparisons of animal response and performance between treatments, treated farms with control farms or treated and control farms with research stations (Ahmed et al., 1985; Gryseels, 1984). Controls should only be compared with treatments if sufficient experimental control was possible. If uniformity was lacking, comparisons are unreliable (Oxley, 1985).

While the analysis of means is important to the researcher, it may not be relevant to the farmer. Lightfoot (1984) suggests that recommendations cannot be based on the analysis of means. Farmers are more interested in the range and stability of animal performance than average production. He also postulates that mean data may not be reliable and the accuracy is questionable. To analyze data in terms of the farmer's criteria, Lightfoot (1984) recommends the use of one of three methods. While the methodologies are oriented towards crop research, they may be applicable for livestock studies. First, interquartile ranges and confidence intervals can be used to estimate the range of outcomes under different conditions. The conditions where new technologies perform poorly and where they perform well are known. For animal research, these conditions may be different levels of management or resources. With this method, the use of experimental failures can be incorporated into the analysis and become useful data. Second, a method called the modified stability analysis evaluates two or more treatments from a range of environments in order to partition farmers into recommendation domains. The variation of treatments between different environments is estimated by regressing outcomes against an environmental index. Calculation of an environmental index involves averaging the effects of all treatments at one location. If there are more than ten locations, each location becomes an index. When there are fewer than ten, the index is derived from each replicate. By combining response curves from two treatments, environments are identified where one treatment is superior to the other. There are several difficulties when using this type of analysis for livestock trials. Locations may not be appropriate for determining environmental indices. Within the research site, differences in management may define an environment more clearly than location. Indices could be defined as different management levels. With agronomic studies, the replication of many treatments at different locations allows for the creation of many environmental indexes. The wider the range of environments that can be incorporated into the analysis, the better the results. However, the nature of OFLR limits the number of environments or management systems studied and the number of possible replications. Increased documentation or use of the modified stability analysis is needed to further refine the technique for livestock trials. Third, Lightfoot recommends that the risk of failure to those who adopt the technology could be estimated by calculating the probability of failing to reach specified disaster outcomes. For example, during a drought or disease outbreak, what is the likely outcome and the associated probability of occurrence for each treatment? Like the modified stability analysis, no documentation is available to fully evaluate the methodology.

Economic evaluation. Increased biological production is not enough to recommend a technology (Tully et al., 1985). A comprehensive economic

analysis is also needed to evaluate an intervention. To fully address the complexities of economic analysis is beyond the scope of this paper. An excellent, practical bulletin by Perrin et al. (1978) discusses major economic parameters and outlines specific methodologies when conducting on-farm research.

A partial budget analysis is used most often when conducting OFLR (Zandstra, 1985; Sidahmed et al., 1985; Ahmed et al., 1985; Tully et al., 1985). Technical data and market prices are combined to evaluate treatment effects. For example, Tully et al. (1985) evaluated treatment effects during grazing trials by estimating net output value. Actual output can be determined on a per flock basis or per unit of investment. Tully suggested that the output per flock is more important to the farmer because the level of investment is usually low. Cost/benefit analysis has also been used for OFLR (Sidahmed et al., 1985). Marginal rates of returns are compared to the incremental costs of production in order to evaluate economically promising treatments. However, the analysis is based on minor changes in the system and fails to consider the interrelationships between resources, nutrition, health, etc. (Sidahmed et al., 1985). Whole system modeling is often suggested to evaluate such interrelationships. But modeling may not return costs in time and effort (Zandstra, 1985). It's recommended that modeling be done only when evaluating new production systems and not for normal research (Ahmed et al., 1985). The simulation of economic activities has been used successfully when abnormal conditions (i.e., drought) affected experimental results (Ahmed et al., 1985). Other documented methods include sensitivity analysis of price variations, time series analysis and the evaluation of the timeliness of inputs (Ahmed et al., 1985; Starkey and Apetofia, 1986).

The use of empirical data and adoption. The predictive approach, as discussed above, allows the team to logically evaluate new technologies within the farmer's environment. By collecting data under experimental conditions, the research team attempts to predict how farmers might react to a new technology. However, strict reliance on experimental data for evaluation eliminates an important component of on-farm research. Farmers' observations provide valuable input when evaluating a farming system, a new technology and the experiment (Atta-Krah, 1985; Yackout et al., 1985; Sidahmed et al., 1985; Ahmed et al., 1985; Bunderson et al., 1985). When conducting researcher managed trials, empirical data from farmers can identify problems, constraints and opportunities from farm experiences not recognized by the team. Jaeger (1984) used empirical data to evaluate effects of animal traction. A series of cross-sectional and time series observations were recorded across a sample of small farmers. Informal interviews formed the basis for collecting farmer's opinions about treatments in a study by Tully et al. (1985). It has been suggested that the best way to evaluate a technology is by allowing farmers to decide which treatments are acceptable. For example, Starkey and Apetofia (1986) suggest that the only way to assess the ability of animals to perform work through the farming season, while using resources which are available and affordable, is to use the farmer's evaluation. The absolute measures in animal traction are meaningless. Only farmers are able to judge animal performance and identify constraints. Farmers evaluated the treatment in terms of changes in survivability and weight gains. The interpretation of farmer's observations should be done with caution. Farmers develop opinions based on their own experiences and self-interests. Likewise, opinions are easily confounded when conducting trials on farmer's land (Tully et al., 1985). They may like the technology but dislike the inconvenience of conducting a trial. Before making far reaching recommendations based on empirical

data, the team must be confident that results come from a typical environment, climate and a representative group of farmers.

Besides the use of empirical data to evaluate treatments during experiments, estimates of acceptability provide an additional analysis for farmer managed trials. The rate of acceptance is a true measure of a technology's success or failure. When farmers are willing to spend their own money and time on a treatment while paying full cost, the innovation can be considered successful under the current conditions (Yackout et al., 1985). Given the large investment in capital and labor associated with changes in livestock management, adoption of a new technology should increase confidence in the treatment's effect. An acceptability index is one method used in crop research to test adoption (Shaner et al., 1982). The index is calculated by multiplying the percentage of farmers who adopt the new technology by the percentage of the affected crops on their farm and dividing the product by 100. An index of 25 is large enough to warrant the treatment's recommendation. If crops are replaced with animals in the equation, the index may become only a percentage of adoption. Small farmers with livestock usually are unable to implement a technology on a portion of their flock or herd. Management practices or small numbers of animals will not permit partial changes. Therefore, the percentage of adopting farms is multiplied by a coefficient of 1. The index has limitations when used with livestock. An alternative might be to estimate the percentage of animals managed under the new technology within the research site. However, results derived from percentages should be evaluated carefully. For example, a 50% adoption rate by farmers is not the same as 50% of the animals. All of the adopting farms may be of one type and manage less than 10% of the animals in a region. Likewise, all of the animals utilizing the new treatment may be owned by a few farms. The acceptance of new treatments may lead to increases in animal numbers. Changes in flock or herd size could be an alternative parameter to measure acceptability. Atta-Krah (1985) measured flock size when conducting trial on free ranging sheep and goats. An increase in numbers demonstrated a successful treatment. Rate of adoption was used by Bunderson and Cook (1985). When measured over time, the analysis provides information on the perceived risk or the availability of new capital and labor.

Conclusion

After reviewing experiences from OFLR, one conclusion becomes evident. Because OFLR is relatively recent and methodologies are still developing, no clear set of guidelines exists. With such diversity and variation found at each research site, the reliability of research methodology recommendations becomes questionable. Successful farming systems research depends on the team's ability to clearly define research objectives and understand the principal concepts of systems research. Rather than follow specific guidelines, the modification of experimental procedures to "fit" the research environment and objectives will yield improved results. For example, much OFLR is oriented towards determining statistically significant differences between treatments. If much variation exists between experimental units, tests for significance are best left to on-station research. It is recommended that OFLR be limited to farmer managed trials and use measures of acceptability as test parameters. The uniqueness of each research project makes the selection of experimental methodologies an art rather than a science.

Even with such diversity, new experimental techniques can provide future research with relevant information. Therefore, publications of OFLR should include explicit descriptions of all methodologies used. Not only should

techniques in biological testing be included, but all methodologies unique to farming systems research. Sampling techniques, farmer survey methods and site selection are but a few areas which need further development. Likewise, additional study into the effectiveness of research methods is needed. Examining the success of past research remains a low priority. Yet, the improvement of methodologies comes only from evaluating past performance. The combination of increased communication between livestock researchers and the testing of research techniques will further advance the effectiveness of OFLR.

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